following sounds in answer: *eff equals emm aye!* Do not tolerate the mere reading of an equation in answer to a physical question.

Do not tolerate vague statements. It is physically meaningless to say, for example, that acceleration is gain of velocity divided by time." The proper statement is that the average acceleration *of a body during a given time* is equal to the velocity gained *by the body during the given time* divided by the time. It is meaningless to say that "density is mass divided by volume." The proper statement is that the density of a body is equal to the mass of the body divided by its volume.

Require the student to make every statement of definition, every statement of principle, every explanation of an equation, etc., as relating explicitly to a particular condition or thing.

The natural desire for brevity of statement is often allowed to go much farther than the elimination of the important element of explicitness as above pointed out, and lead to complete obscurity of meaning as illustrated by the following example: A string 10 feet long is tied to a post and a force of 5 "pounds" is exerted on the post by pulling the string. This force certainly "acts through a distance of 10 feet," and, the work done is 50 foot-"pounds" because "work is done when a force acts through a distance." This argument is found to be acceptable to about 60 per cent. of the men beginning a college course in mechanics! No! *Work is done when a body on which a force acts moves in the direction of the force,* and no dictionary ever defined the word *through* in a way to justify the use of the word to abbreviate this 18-word statement as it is usually abbreviated in the study of physics (?) in school and college. Language has been developed as a medium for dickering, quarreling and love-making, and language as used in precise physical specifications is always more or less awkward and more or less strained; but it is a serious mistake to obviate these things by using meaningless expressions and phrases.

I have never talked with an electrical engineer who retained any helpful knowledge or understanding whatever from the study of electrostatics in his college course in physics; and every electrical engineering teacher will tell you that he cannot count on any knowledge or understanding, even incipient knowledge and understanding, of electrostatics among students who have just finished their college course in physics.

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REQUIREMENTS OF A MONOGRAPH ON THE CHEMISTRY OF CELLULOSE[1]

IN a seminar devoted exclusively to the chemistry of cellulose certain topical assignments were made to the students, who, after a careful and critical survey of the literature, reported their findings. The course served to emphasize a number of sad facts that are undoubtedly known to all students in the field of cellulose chemistry. We were impressed by the enormous number of undigested, uncorrelated facts that had been amassed apparently as a result of technological studies. We were further impressed by the relatively small number of fundamental studies (bearing the earmarks of painstaking critique on the part of the investigator) that had a direct bearing on the constitution of cellulose, and by the amazing method of presenting these facts in our best English text. It became quite evident as our course proceeded, that there was a lack of vision in the interpretation of noteworthy results in the literature; that little attention had been paid to the methods employed or judgment exercised by investigators in the experimental portions of their work; that scant attention had been given to the correlation of isolated experimental data, and that little differentiation had been made between qualitative and quantitative data in the formulation of hypotheses. To present the case briefly—it became very apparent that a *critical* monograph in the English language was little less than a necessity. Since the close of our seminar, Heuser's new "Lehrbuch der Cellulose Chemie" has appeared, and this splendid work will receive further mention.

A few examples will serve to illustrate the various points previously raised. Take the

[1] Read at the meeting of the American Chemical Society, New York, September 9.

case of the scientific investigations stimulated by the mercerization reaction. Since 1850 (or thereabouts) a number of investigators including Gladstone,[2] Vieweg[3], Thiele,[4] Cross,[5] and others have assigned various formulas to the compound (or compounds) that had been formed between cellulose and sodium hydroxide when concentrated alkali acted upon cotton. The existence of such compounds was disputed by Hubner and Teltscher[6] and later by Leighton.[7] From a hasty review it would appear that the existence of a definite compound between cellulose and sodium hydroxide had never been demonstrated, and that alkali cellulose may perhaps be attributed to adsorption phenomena. Nevertheless, Leighton's work has not affected our interpretation of the constitution of viscose, which presupposes a cellulose alcoholate, ($C_6H_9O_4ONa$ or some similar compound) which then reacts further with CS_2 to form at the outset of the "ripening" process sodium-cellulose-xanthogenate, which gradually hydrolyzes with the loss of NaOH and CS_2 until cellulose is regenerated. It remains possible of course that the xanthogenate reactions given in our texts accurately represent the formation of viscose—and yet, in the light of Leighton's investigations it is disconcerting to note the quiet assurance and certainty with which this explanation of the xanthogenate reaction is generally accepted.

A far more striking example of the lack of critic and indifference with which experimental details are treated in our modern cellulose literature is to be found in the case of the hydrolysis of cellulose to glucose. Our literature has been replete with confident statements that within the limits of experimental error, cellulose is quantitatively hydrolyzed to d-glucose:

$$(C_6H_{10}O_5)_n + nH_2O \rightarrow nC_6H_{12}O_6.$$

Irvine and Soutar,[8] however, have justly shown that this claim has always been made on the grounds of questionable or incomplete experimental evidence, and that in no case was dextrose or a dextrose derivative isolated in any amount approaching the theoretical yield. There is no object in reviewing the work of Flechig,[9] Schwalbe and Schultz,[10] Willstätter and Zechmeister,[11] or Ost and his co-workers.[12] Such a review would either show indirect evidence or incomplete evidence regarding this very fundamental reaction. *It is only within the past year* that Irvine and Soutar themselves[8] have shown that the above equation is substantially correct and that at least 85 per cent. of dextrose is formed when cotton cellulose is hydrolyzed. They failed to account for less than 15 per cent. of the hydrolysis products. Irvine's work is noteworthy in that he isolates his compounds in a state of analytical purity. His experiments are all quantitative and all of his products are definitely identified. The judgment and critique exercised throughout this study are remarkable, and the research must stand as a classical one. It presents a marked contrast to the previous investigations in the same field. It is furthermore interesting to note that whenever the cellulose-dextrose relationship has been brought into question, the question has not been raised as the result of some investigators' lack of critique, but because of certain reactions (like the bromomethyl furfural reaction of Fenton and Gostling) which were themselves far from quantitative, and the mechanism of which was not fully understood.

During the course of the myriad cellulose investigations that have crowded our literature, a number of so-called "compounds" of cellulose have been isolated and characterized. Let us examine briefly the case of the "oxycelluloses," compounds obtained by the oxidation of cellulose. There is no necessity of reviewing the methods of formation, or the properties of these substances. If we accept Hibbert's view of the constitution of cellulose, the oxidation of cellu-

[2] *J. Chem. Soc.*, *5*, 17 (1853).
[3] *Ber.*, *40*, 3876 (1907).
[4] *Chemiker-Ztg.*, *25*, 610 (1901).
[5] "Cellulose," p. 23.
[6] *J. Soc. Chem. Ind.*, 28, 641 (1909).
[7] *J. Physical Chem.*, *20*, 32 (1916).
[8] *J. Chem. Soc.*, 117, 1490 (1920).
[9] *Z. physiolog. Chem.*, 7, 523 (1883).
[10] *Ber.*, 43, 913 (1910).
[11] *Ber.*, 46, 2401 (1913).
[12] *Chem. Ztg.*, 34, 461 (1910).

lose might run the entire gamut of hydroxyaldehydes, hydroxyketones, hydroxyacids, ketoacids, etc., that could result from a product having two secondary and one primary alcohol groups for each six carbon atoms. Since the oxidation reaction is not infrequently accompanied by hydrolysis, the possible number of products is accordingly increased. We have here a limitless field for speculation, and can think of an indefinite number of oxycelluloses, depending upon the type of oxidizing agent, the conditions of oxidation, on the amount of oxidation product adsorbed on the residual cellulose, and possibly on other factors as well. It is quite evident that we can hardly hope for a homogeneous substance, and it is obvious that oxycellulose is a very vague and illusive term. It has no particular chemical significance and yet it persists in our present-day text-books on cellulose. The term "hydrocellulose" and "cellulose hydrates" enjoy a similar distinction. The former has been shown to be a mixture of hydrolytic degradation products of cellulose and cellulose itself. Whereas the latter (in many cases at least) appears to be cellulose itself—changed physically it is true—but hardly meriting the term applied to it.

I might continue further and point out the incongruities in our literature on lignocellulose and the other so-called "compound celluloses," or the ever-shifting meaning of the term cellulose itself when applied to a substance other than the seed hairs of the cotton plant. Further reference is unnecessary however. It is quite clear that we have certain chemically meaningless but highly respected terms in our cellulose literature, that the results of numberless experiments remain uncorrelated with the properties of the typical cellulose and that our cellulose literature is becoming increasingly unwieldy. I hasten to add, however, that in certain quarters this lack of critique and cohesion is rapidly being remedied—and it is in these quarters that our monographers should seek their inspiration.

To my mind, the primary objects of any monograph on cellulose are: (1) to stimulate further research along scientifically profitable channels; (2) to present the literature in such a way that the reader may have a reliable means of knowing whether or not previous statements can be accepted without reservation; (3) to present the data with a view towards giving the reader a comprehensive survey of the cellulose field without losing him in a maze of detail; (4) to pave the way for a more satisfactory definition of the term cellulose.

To gain these objectives, the author should remain uninfluenced (whenever necessary) by the orthodox procedure of previous writers, and should approach his problem in an essentially modern spirit. He must effect a liaison between some of the hitherto isolated facts in cellulose chemistry. He should use the greatest critical ability at his command, and give weight to results of those investigators who have used proper critique in their own work. Furthermore, he should select his material in such a way that with slight revision and proper additions, the work would remain a standard book of reference for a number of years to come.

It is quite possible to cleverly compile into a scholarly treatise (or series of treatises) a mass of detailed information—but such a volume would hardly meet our requirements. We need a critical compilation—suggestively written—that will give due weight to important qualitative reactions of cellulose and to the results of quantitative studies as well. The danger of formulating hypotheses on the basis of purely qualitative reactions should be constantly kept in mind. Articles in which unwarranted conclusions have been drawn without sufficient data, or in which the critic of the investigator is questionable should be subordinated or entirely deleted. Many of the vague terms now in common usage in the cellulose literature should be redefined or excluded.

Technological aspects of cellulose chemistry deserve no place in such a monograph. Paradoxical as it may seem, such a volume should in the end prove more serviceable and suggestive to the cellulose industry than would one which is diluted with references to the technological processes. This is especially true since we are already in possession of some noteworthy monographs in which these

technological processes have been compiled with the greatest patience and industry.

At the outset it would be advisable to publish only one monograph dealing with cellulose chemistry. It would be unfortunate if the society published a series of separate monographs on such subjects as (let us say) cellulose hydrates or oxycellulose. If one monograph cannot be made the joint work of two authors (an organic and a physical chemist), it might be well to have two monographs, one on the "chemistry of cellulose," and one on "cellulose as a colloid." Needless to say these books should supplement each other. I can not help feeling that an extended series must lead us into the same difficulties that we have encountered in the past, and I do not think that such a series would prove a good investment. Certainly the details in a number of volumes of an extended series would be obsolete in a comparatively short time. A carefully written volume of 300–400 pages with a properly classified bibliography should serve our purpose better than would an entire series.

I claim no originality for the ideas set forth nor are they Utopian. They form the basis of Heuser's recent "Lehrbuch der Cellulose Chemie." From the standpoint of the organic chemist, Heuser's Lehrbuch is the best monograph in its field. Unfortunately it was published several months too early to include the results of Hibbert's and Irvine's work on cellulose and Haworth's work on cellobiose, and it suffers accordingly. Heuser has written with a clear vision of the requirements of a modern monograph on cellulose. His writing is singularly free from circumlocution and from perplexing detail. He develops his subject matter clearly and logically. He has, however, omitted full reference to the modern work on the colloidal chemistry of cellulose, an oversight that should be corrected in any American monograph.

Summary.—(1) We require a monograph on the chemistry of cellulose that briefly and critically presents the most noteworthy results in the cellulose field. (2) The monograph must be more than a painstaking compilation. (3) It should carefully select the literature dealing with the most important reactions of cellulose as well as the results of the more recent researches on the physical properties of cellulose. (4) It should be written to stimulate fundamental research. (5) It should be free from inconsequential or meaningless terms and hypotheses.

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EUGENICS—THE AMERICAN AND NORWEGIAN PROGRAMS

Dr. Jon Alfred Mjøen, recognized by the Norwegian Government as the leader in eugenic and hygienic reform, issued from the Winderen Laboratorium, May, 1908, the following "Program for Race Hygiene":

Negative Race Hygiene. (*a*) *Segregation* (negative colonization system) for feeble minded, epileptics and similar physically and mentally crippled individuals, obligatory for drunkards, habitual criminals, professional beggars and all who refuse to work. (*b*) *Sterilization.* No compulsory sterilization in general. *Certain types* of criminals who wish to escape segregation should be given an opportunity to be sterilized.

Positive Race Hygiene. (*c*) *Biological Enlightenment.* Education of women in school and university should be changed from the present masculine system to one adapted to the female intellect and mind. Biology (renewal of the family), chemistry (nourishment of the family), and hygiene (protection of the family) should be chief subjects (obligatory), from the preliminary class in the boarding school to the university.—Race biology in school and university institute for genealogical research. State laboratory for race hygiene. (*d*) *Tax-, Wage-* and *Colonization*-system in favor of families, maternity insurance and other protective measures of prenatal kind. Positive colonization system. Regressive tax and progressive wage system for heads of families.

Prophylactic Race Hygiene. (*e*) *Combating racial poisons:* industrial poisons, especially lead and lead compounds; pathological poisons, especially syphilis; narcotic poisons, especially alcohol. (1) Prophylaxis of race illnesses and race anomalies as a state function. (2) Health declaration before marriage. (3) Class-system and progressive taxation for alcoholic liquors. (*f*) Crossings between